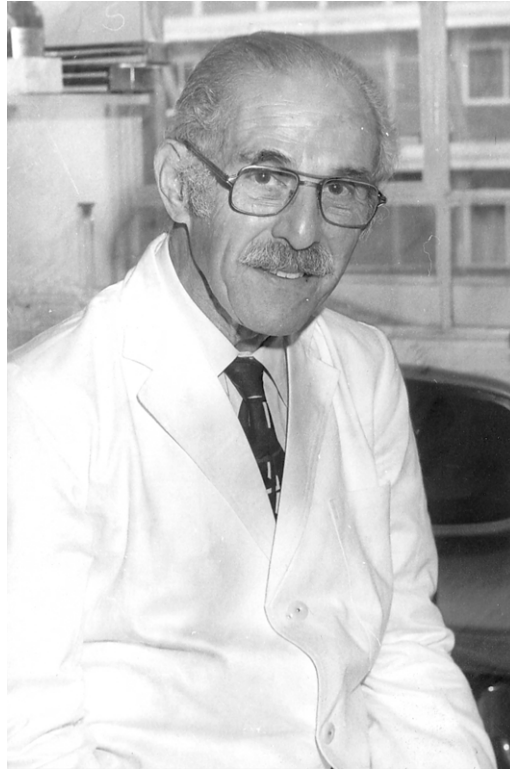


Arthur G. Steinberg, 1912–2006

Trefor Jenkins



Arthur G. Steinberg

Arthur G. Steinberg will probably be remembered principally for his work on the genetics of the immunoglobulins, but there was a good deal more to him than that. Kindly, wise, cultured in the widest sense, he was an inspiring teacher and intolerant of pretension but even more intolerant of intolerance. He was born in 1912 in Port Chester, then a rural suburb of New York City; soon thereafter, his family moved to Manhattan, where Arthur was raised and educated. His mother was born in Brooklyn and was educated at a grammar school; his father, born in Riga, Latvia, had been apprenticed to a house painter and emigrated to the United States in 1903 at the age of 19 years. His education had been limited, but he taught himself Russian and English and read widely. Arthur was the first in his family to attend college. After being educated at Stuyvesant High School, he then started night school at City College of New York (CCNY). He was work-

ing as a “runner” or errand boy for a Wall Street brokerage firm at the time of the great stock market crash. Full-time study at CCNY followed, and, after graduating in 1933, Arthur applied to study embryology at Columbia University’s graduate school. His application was successful, although he was warned that, because he was Jewish, he would not get a job. He enjoyed the embryology course but “it could not compete with Professor L. C. Dunn’s genetics course”¹ which, although it was his first exposure to genetics, “fascinated, intrigued, overwhelmed”¹ Arthur. (These quotes throughout the article are from the work of Steinberg¹ unless stated otherwise.) He determined to become a geneticist. Dunn’s human qualities also impressed Arthur, who saw Dunn as a man “interested in the welfare of the students who vigorously opposed attempts to bring Nazi representatives to the campus.”

All his life, Arthur Steinberg embraced many of the

Department of Human Genetics, National Health Laboratory Service, University of the Witwatersrand, Johannesburg
Am. J. Hum. Genet. 2007;80:1009–1013. © 2007 by The American Society of Human Genetics. All rights reserved. 0002-9297/2007/8006-0002\$15.00
 DOI: 10.1086/518391

views and attitudes toward race and racism that he had seen and admired in Dunn. Steinberg's sympathy for the disadvantaged in society was made clear to all. On one of his early visits to South Africa, in the mid-1960s, he expressed a wish to meet with a champion of the rights of "blacks" in the country, Professor Eddie Roux, Head of Botany at the University of the Witwatersrand. By that time, Roux had been "banned" by the Nationalist Government, was not permitted to enter any place of learning, and was prevented, by law, from meeting freely with more than two people at a time. Hence, he was not able to attend a party in my home, given in honor of Arthur. Roux died the following year from aplastic anemia, probably caused by the insecticides used in his poorly ventilated garden greenhouse, where he was conducting breeding experiments on *Cosmos*.

Arthur's interest in South African politics persisted to the end of his life. He first came to South Africa in 1964, after hearing that the Asiatic Indian population of Durban had the highest rate of connubial diabetics (both marriage partners affected) in the world. Because of his interest in population genetics, I sought him out, and our friendship dates from that visit. I am grateful for his friendship, advice, and encouragement over the past 40 years. The project on diabetes never took off, but our collaboration on population genetics began then (I did a postdoctoral year in his laboratory in 1968) and continued until Arthur's retirement in 1982. Our friendship endured right up to his death in May 2006.

During his graduate studies at Columbia, Arthur was appointed as Dunn's laboratory assistant to help with mouse T-locus studies that led, among other discoveries, to the elucidation of "meiotic drive." But Arthur preferred working on *Drosophila* and made important contributions on crossing-over rates of chromosomes while researching Bar Eye. He spent a number of summers, during the mid-1930s, working as a graduate student at Cold Spring Harbor (CSH), learning from lectures and from informal discussions with many leaders in the field. His meeting at CSH with Boris Ephrussi led to Arthur's becoming his laboratory assistant and using the technique that Ephrussi and George Beadle had developed to transplant *Drosophila* larval imaginal disks. This led to an invitation from Ephrussi for Arthur to work in Ephrussi's Paris laboratory in the (northern) summer of 1938.

Arthur received his Ph.D. in January 1941 for research on the development of transplanted Bar Eye imaginal disks in *Drosophila*. Sixteen months previously, he had received a grant that enabled him to attend the International Congress of Genetics, held in Edinburgh in August 1939—just prior to the outbreak of World War II. As the war clouds were gathering, hurried departure on a U.S. freighter (*City of Flint*) that had never carried passengers before necessitated the hasty construction of rigged beds for the 11 American passengers, who, besides Arthur, included George Beadle and James Neel. The ship sailed the day before the declaration of war and, on the second day

out, answered an SOS from the *Athena*, which had been torpedoed and sunk just hours after war was declared.

The survivors, 200 (according to Neel²) or 290 (according to Steinberg¹), were taken on board, and living conditions were poor until the 200 or 290 "overbooked" *Athena* passengers disembarked at Halifax, Nova Scotia. The "regular" American passengers sailed on to New York.

In spite of the disrupted nature of the Edinburgh congress, it is noteworthy for the production of the "Geneticists' Manifesto" drawn up by Hermann Joseph Müller.³ This constituted a serious indictment of the implementation of eugenics policies legalized and implemented in the United States and already being introduced (with even greater ferocity) in Nazi Germany. There were seven "original signers" of the Manifesto and 14 other geneticists, including Arthur, who also signed the Manifesto.

His first job was as an instructor in the Department of Genetics at McGill University (1940–1944). The chairman, Professor Leonard Huskins, who had met and had enjoyed many discussions with Arthur at CSH before the war and was a staunch supporter of Arthur, appointed him in spite of administration objections on the grounds of Arthur's Jewish ancestry.

When the United States entered World War II in 1941, Arthur was exempted from military service because of a back problem that plagued him throughout his life, but he made a contribution to the war effort by working with a chemist on a project aimed at increasing the yield of a high explosive, known as "RDX." The result was an increase in the yield from 50% to ~95%. The United States was unwilling to share the enrichment technique with their Soviet allies despite the fact that dozens of ships and scores of men were lost exporting RDX (and other supplies) to the Soviet Union, via Murmansk. The chemist working with Arthur, in exasperation, gave the method to the Russians. He was jailed as a spy, and the fact that Arthur had worked with him meant that Arthur was not rehired by McGill when his period of "service" with the Navy's office of Scientific Research and Development (OSRD), 1944–1946, came to an end. He spent 2 years, 1946–1948, at the Fels Research Institute of Antioch College but, beginning then, he was repeatedly investigated by the FBI and many congressional committees, continuing until early 1956. These hearings were accompanied by much publicity, and this explains why he moved from position to position during that time. He was extremely happy at the Mayo Clinic for the period 1948–1952, where he researched, in collaboration with clinicians, the genetics of psoriasis, cancer of the jejunum, chronic relapsing pancreatitis, the Wiskott-Aldrich Syndrome, and adult-onset diabetes. Studying the families ascertained by Dr. Russell Wilder, Arthur showed, among other things, that the phenomenon of "anticipation" in diabetes is simply a statistical artifact and not a biological event. The Mayo Clinic fired him in 1952—after 4 years of very productive research there—following a House of Representatives Committee hearing at which Arthur was required to testify,

even though a press release by the committee stated that “there was no reason to suspect [him] of wrongdoing.” From 1952 to 1956, he worked at the Children’s Medical Center, Boston, in what is now known as the “Dana-Farber Cancer Center.” There, he did a classic study of the families of 249 of the Center’s patients with leukemia, showing that there was no evidence of increased incidence of the disease among the patients’ relatives. There were also classic linkage studies of blood groups, cystic fibrosis, and hypercholesterolemia.

It was while Arthur was in Boston that he made contact with the Hutterites, a sect of Anabaptist Protestants who live a communal life on farms in North Dakota as well as in Canada. This initial visit was to enlist the cooperation of the community and to gain their permission to perform various studies of them, but it was only after moving to Western Reserve University, later in 1956, that he performed the actual field work.

Arthur had been forced to relinquish his job in Boston after the publicity created by a 1956 government committee, the Un-American Committee of the Senate. Luckily for Arthur, the then-president of Western Reserve University (it soon became and remains Case Western Reserve University [CWRU]), Dr. John S. Millis, was one of the “rare individuals at that time who believed in our Constitution and acted on this belief, despite the hysteria generated by the demagogues of the Un-American Committees of the House and Senate.” Arthur accepted the job at Western Reserve in the Biology Department and remained there until he retired in 1982. He built a strong laboratory and saw the Biology Department grow and strengthen.

When he moved to CWRU in 1956, Arthur assumed responsibility for a genetics counseling (“heredity”) clinic. He recalled how difficult it was to deal with some young couples. “They’d had a baby with some awful business, and all you could say was that they had a twenty-five percent risk in its recurring. Couldn’t do much of anything for them. I’d come home after clinic and my wife would take a look at me and say, ‘You had heredity clinic today.’”⁴ He likened the stress to that on the physicians in Boston who specialized in childhood leukemias.^{4(p367)}

His research on immunoglobulin genetics brought him world fame. There was a continuous stream of Ph.D. graduates researching both laboratory and statistical genetics, as well as those who worked on material gathered by Arthur in field work among the Hutterites in 1958–1964, soon after his appointment at Western Reserve.

The research on the Hutterites conceived in 1956 and initiated by Steinberg in 1958 was pioneering: it was the first large-scale genetics study performed on a large human population isolate. Alice O. Martin, a Ph.D. student of Steinberg’s in those early days (in a keynote address to an “Anthropology, Genetic Diversity and Ethics” workshop at the Center for Twentieth Century Studies, University of Wisconsin, Milwaukee) has recalled the Steinberg philosophy regarding such field work. She describes it as, first and most important, “respect the culture” of the people

being studied. All the women wore long dresses—so Martin and her female colleagues would not wear miniskirts! The cooperation of the leaders would be gained first, the research goals and the use to which the data would be put would be clearly explained, and preservation of the confidentiality of all the participants would be guaranteed.

According to Martin, Arthur tried initially to conceal the identity of the population he was studying: in his early reports, he did not refer to them as Hutterites but called them “H-Leut.” “Leut” means “people,” and the Hutterites refer to themselves as “Leut.” Why conceal their identity? They were a pacifist group and spoke German; this led to awkward situations at the time of the World War I, arousing suspicion. The Hutterites are often rich farmers and, because of jealousy, not universally popular with their neighbors.

The very good relations between researcher and study population continue to the present, when the studies are being performed and coordinated by Carole Ober at the University of Chicago; asthma, fertility, and other health related topics are still being researched, and she now exploits molecular-genetics techniques in her work. The research subjects continue to be treated “not merely as subjects, but more as somewhat limited partners,” cooperative research projects as envisaged by Arthur Steinberg back in the 1950s. The research subjects still appreciate receiving from the researchers a printout of their family pedigree and their blood groups—a practice introduced by Arthur Steinberg nearly 50 years ago. It is plain that researchers working on the Hutterites who follow in the Steinberg tradition, largely his former students, never indicate the name of the population in the titles of their papers but simply say “human isolate” or “founder population” and didn’t even give the country of origin!

The “hounding” of Arthur Steinberg by U.S. government committees of the House and Senate and, in particular, the Un-American Committee in 1956 had a destructive effect on his career in science for over 10 years. That he succeeded, in spite of this, bears testimony to his strength of character and commitment to his science. A less resilient individual would probably have retreated from science altogether. It is particularly remarkable that, after failing to gain reappointment to a succession of posts, all of which he had filled with distinction, he still persevered, finding new posts through colleagues and friends who believed in his innocence and his strength in the emerging discipline of human genetics. He was elected to the Board of Directors of the American Society of Human Genetics in 1955 and became its president in 1964.

Until I read “Much Ado About Me”—an autobiography by Arthur G. Steinberg¹—I had no idea of the extent of the suffering he had endured as a result of his problems with the Un-American Committees. “He had made an occasional reference to the McCarthy era, of course, but he had not allowed the episode to interfere with his scientific career; a less determined and less ‘compleat’ personality might well have given up on the system or else have be-

come embittered to the point of paralysis and withdrawal. It says much for Steinberg the man that he ‘weathered’ the crisis and continued in a successful career in science, which has carried such a high profile.”⁵ I have encountered only one human geneticist who has made a reference to Arthur’s alleged involvement in “un-American activities” as if he seemed to believe that there was some truth in the allegation.

Arthur distinguished himself as editor of leading publications in the discipline of human genetics. He was the fourth editor of the *AJHG* (vol. 8, 1956, through vol. 13, 1961), explaining that he gratefully accepted the appointment “as a vote of confidence by my colleague, because it came at a time of unpleasant publicity.”^{1(p255)} He was also founding editor of *Progress in Medical Genetics*, serving as the sole editor for its first volume (1961) and senior editor for the subsequent 15 volumes, with Alec Bearn as coeditor, before they both relinquished the editorship after Arthur’s retirement from academia. Bearn, in the work of Fraser et al.,⁵ came to appreciate Steinberg’s “extraordinary editorial skills, which enabled him to improve substantially the articles submitted.” John Opitz, in the work of Fraser et al.,⁵ paid tribute to him, considering him to be one of the greatest editors in the field of human genetics. During his editorship of *AJHG*, Arthur became so concerned with the quality of some of the reviews that he “ruled that referees would no longer be anonymous. The reviews were greatly improved, but the referees were not happy with my objection to anonymity. The policy was abandoned by my successor and has remained so.”^{1(p255)} He was also contributing editor of *Transfusion*, contributing editor of *Vox Sanguinis*, and a member of the editorial boards of *Immunogenetics* and *Acta Anthropogenetica*.

Arthur was named the Francis Hobart Professor of Biology in 1972 and a Case Institute Centennial Scholar in 1980 “in recognition of distinguished scholarship and many contributions to the field of human genetics, your service to professional societies, foundations, and government agencies, your service to the local community and to your numerous students and colleagues.” In 1999, he was recipient of the Frank and Dorothy Humel Hovorka Prize in recognition of “exceptional achievement by an active or emeritus member of the faculty” at CWRU, “whose exceptional achievements in teaching, research and scholarly service have benefited the community, the nation and the world.”

Arthur traveled to all the inhabited continents, attending scientific meetings and lecturing or conducting research on populations as diverse as the Ainu of Japan and the San of the Kalahari Desert in Botswana. He accompanied me when we visited the !Kō (in 1967) and the !Kung (in 1975). On the latter trip, we stayed with Ed Wilmsen in his comfortable camp at /ai/ai, but the earlier trip was much rougher when we camped in the open air, sleeping on stretchers, and often suffered a shortage of water. Arthur was first up in the morning when the tem-

perature reached the freezing point, and I recall his saying that he had not been as dirty since he was a kid growing up in the back streets of New York! He had life-long problems with herniated intervertebral disks, necessitating sequential surgical removal of a number of them; driving in a Land Rover over the extremely rough sand roads/tracks in the Kalahari caused him great discomfort, which he stoically endured—all in the cause of science!

Arthur was highly competitive in some ways but was driven to be open and generous, welcoming visiting scientists to his laboratory; he was angry with one colleague who, after visiting his laboratory, shared or published some information he had gleaned on his visit, without acknowledgement. Nor did he appreciate Watson’s *The Double Helix* when it appeared in 1968; he felt that the author’s conduct in obtaining information regarding Rosalind Franklin’s research was ungentlemanly, if not unethical! He was quick to accept the “double helix” model for the gene, and, when teaching students, he modified his definition of the gene as new information about it and its functioning was elucidated. We all thought he organized and ran his laboratory in an old-fashioned, autocratic way, and he was not himself skilled in biochemical techniques. If a piece of apparatus was not functioning smoothly, we used to dread the prospect that Arthur might attempt to “fix it!”

Arthur and his wife, Edith, were extremely hospitable and threw wonderful parties at their home. Guests who were Edith’s friends or acquaintances (her stage name was Edith Owen) would refer to Arthur as “Dr. Owen”—much to his amusement; his pride in his wife’s charm and friendliness was evident to all.

Arthur retired in 1982, but, from a tiny office in the Biology Department of CWRU, he continued to write scientific papers and to encourage former students and other colleagues. The loss of secretarial assistance meant that he had to become skilled at using e-mail to remain in contact with former students/friends—in the United States and farther afield. He did tell me once in a letter that, when walking around the campus, he felt like a ghost—nobody recognizing him and he not recognizing anyone either. He and Edith traveled widely during retirement, and Arthur learned to scuba dive at age 70 years. Even as his health began to fail, Arthur would walk three-quarters of a mile to his office each day, using a walker and in all kinds of weather. The last 3 years or so of his life were plagued by a progressive neurological disorder, but his intellect remained razor sharp until a few months before he died. He endured this progressive illness with fortitude, but these latter years of his life must have been very frustrating for him. It says a great deal for the wonderful, loving care that he received from Edith and the dedicated professional health care personnel at Judson Manor that he lived to be 94 years and died peacefully.

Arthur is survived by Edith Owen, his wife of 67 years,

their two children, four grandchildren, three great-grandchildren, and three step-great grandchildren.

Acknowledgments

I am most grateful to the students and colleagues of A.G.S. who generously shared with me some of their memories of a revered mentor and colleague: Alice O. Martin, Elo R. Giblett, Arno G. Motulsky, Ted Kurczynski, George T. Nurse, and Norman Rushforth.

References

- 1 Steinberg AG (1995) Much ado about me. *Am J Med Genet* 59: 250–262
- 2 Neel JV (1994) *Physician to the gene pool: genetic lessons and other stories*. John Wiley, New York
- 3 Muller HJ (1939) Geneticists' manifesto. *J Hered* 30:370–373
- 4 Kevles DJ (1985) *In the name of eugenics: genetics and the use of human heredity*. Alfred A Knopf, New York
- 5 Fraser CF, Jenkins T, Bearn AG, Opitz JM (1995) Arthur G. Steinberg: an appreciation. *Am J Med Genet* 59:245–249